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Theo A. F. Kuipers

KEPLER, NEWTON, EINSTEIN AND THE STRING THEORY

REPLY TO DAVID ATKINSON

Apparently, the string theory raises the interesting question of the extent to which my typology of pure and hybrid research programs is exhaustive. Before I enter into this question, I take the opportunity to use David Atkinson's lucid survey of "pre-string" fundamental physics to indicate some further illustrations of cognitive structures explicated in SiS and ICR.

Brahe-Kepler, Newton, Einstein

Calling Brahe-Kepler's program descriptive is not only adequate, we may even split the contributions of Brahe and Kepler in terms of an "individual" (i.e. individual fact gathering) and an "inductive descriptive subprogram" (SiS, p. 6, ICR, pp. 171-2), for, as Atkinson indicates, Kepler obtained his three laws by inductively generalizing Brahe's observational data. Newton's theory of gravitation represents not only an evident explanatory program, it is also typically equipped with a well-known "evaluation report" (SiS, p. 216, ICR, p. 98) of general successes, notably the laws of Kepler, Galileo and the tides, and (generalized) individual problems, notably the precessing perihelion of Mercury. Moreover, its explanatory successes were not all of the postdictive kind; some included corrective predictive successes (SiS, p. 216, ICR, p. 98). For example, as elaborated in SiS (Sections 3.1.1, 3.3.2), Newton's explanation of Galileo's law of free fall is a pure case of "corrective reduction."

Finally, Einstein's general theory of relativity not only illustrates the notion of being (unequivocally, empirically) "more successful" (SiS, p. 230, ICR, p. 112), viz. relative to Newton's theory, but also that "concept explication" occurs, not only in philosophy and mathematics, but also in the empirical sciences (SiS, pp. 6-9), leading to hybrid explanatory-cum-explicative programs. As a matter of fact, both Einstein's special and general theories of relativity are typically hybrid. The first is hybrid by explicating the notion of

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simultaneity (see Note 3 of Atkinson's contribution for some details) and explaining light propagation and other experiments (unequivocally better than competing theories, SiS, p. 236-237, ICR, pp. 118-9). The general theory is hybrid in that it not only explicates away the "occult force of gravitation," but also outstrips Newton's theory in explanatory success, and not only postdictive (e.g. Mercury's behavior) but also predictive, e.g. the famous light bending which was (more or less) confirmed by Eddington's data. By stating later in his paper that "Einstein's leap was at least in the right direction," Atkinson even seems to support the idea that Einstein's theory is closer to the truth than Newton's, a claim which I see as an evident example that a theory of truth approximation should recognize as a (possible) case of truth approximation (ICR, p. 177). However, I know of Atkinson's reserves in this respect, in particular regarding of the existence of "the truth." As a matter of fact, making the characterizability postulate (ICR, p. 147) explicit and its subsequent relativisation, is something I owe to him.

There is also an interesting global point to make about the three examples of research programs, which should have been mentioned in SiS. Atkinson's short stories about (the core ideas underlying) Kepler's laws and Newton's and Einstein's theory of gravitation and the common practice to speak of their laws and theories instead of their programs, make clear that the development of these programs leading to the final theories was mainly a one-person affair. To be sure, if we focus on the (continued) "separate evaluation" of Newton's theory and on the application of his laws of motion to other forces than gravitation, it typically makes sense to speak of Newton's program that was elaborated by Newton and by others. That is, an evolving (real and virtual) coproduction of several researchers, which is typical of many programs, e.g. string theory, despite the fact that Edward Witten may rightly be called by Atkinson its "most prominent proponent."

The Nature of the String Program

When reading Atkinson's exposition, my attention was suddenly caught by the background music on the radio, when a live Proms violin concerto by Tchaikovsky (concerto in D, opus 25, August 7, 2001) was abruptly halted because of a broken string, which was subsequently repaired by the soloist Vadim Repin himself, accompanied by many humorous comments. If we interpret the breaking of the string as a metaphor for falsification of the string theory and its successful repair as a metaphor for an improved version, perhaps even one that comes closer to the truth, Atkinson's exposition claims that this sequence of events is merely possible *in principle*, not *in practice*. I must confess that I, as a non-specialist, always have doubts about such rigorous

claims as these by specialists. However, for the sake of argument I should like to dwell upon the question of what type of research program it is if HD testing and evaluation will always remain practically impossible. To begin with, I have some reserves regarding Atkinson's hesitation to call it at least in some sense explanatory: "for ... string theory does not make testable predictions". I would certainly qualify his chain of implication "string theory contains Einstein's general relativistic theory of gravitation, Einstein's theory contains Newton's theory as an approximation, and Newton's theory describes [and explains, I assume] quantitatively the falling of 'things', like planets, moons and apples" (p. 100) as a sequence of (partly deductive, partly corrective) reductive explanations of, in the end, experimental results. Hence, string theory has postdictive explanatory successes. To be sure, apparently none of them is an extra success, neither relative to General Relativity nor to the Standard Model. It is the specific claims (e.g. space-time has ten dimensions) and predictions of a host of particles not predicted by the Standard Model that seem impossible to test. Hence, I would call it an explanatory program, albeit of a very special kind in two senses, a negative and a positive one. In contrast to normal explanatory programs, it cannot be experimentally evaluated. On the other hand, as Atkinson strongly emphasizes, the string program has 'unification' of explanatory programs as its chief target, in particular of General Relativity and the Standard Model. In this respect, string theory is similar to other cases, e.g. "the modern synthesis" unifies and surpasses the theories of Darwin, Mendel, and Morgan, but in this case the unifying theory is testable, or at least no less testable than the theories it unifies. Hence, as long as there are no other examples of untestable explanatory programs of a unifying nature, I hesitate to speak of a fifth type. However, I am happy to agree that this is essentially a matter of words, not touching upon the really special character of the string program.

Let me conclude by briefly commenting upon Atkinson's bracketed remark "it is still the case that, for many scientists, 'philosophizing' is put on a par with daydreaming or sloppy reasoning." This reminds me of course of Weinberg's (1993) chapter "Against philosophy." It is regrettable in two respects. First, it illustrates quite convincingly that diehard positivist, that is, (epistemologically) instrumentalist, attitudes cannot only retard but may even become ridiculous. But, second, it also illustrates how one-sided it is to identify positivist philosophy (of science) with philosophy (of science) in general, including the many moderately realist representatives, such as Popper and many others, notably philosopher-scientists, from Einstein to Atkinson.

REFERENCE

Weinberg, S. (1993). *Dreams of a Final Theory*. London: Vintage.